advantageous. The idea was present in Sir John Sinclair's famous "Statistical Account," but has had no recent or adequate embodiment.

THE *Scottish Geographical Magazine* for July contains a translation by Mr. C. E. D. Black of M. Dauvergne's recent journey in the Pamirs, the original paper appearing simultaneously in the *Bulletin* of the Paris Geographical Society. The journey carried out in 1889-90 was a most successful one and opened up some new ground. The geographical results are summed up in four sentences:—(1) That there is another great chain running parallel to the Kuen Lun and facing Kashgaria. (2) That the river in the Tung valley is an affluent of the Zarafshan, not of the Taghdumbash. (3) That the Oxus rises in the great glaciers of the Hindu Kush at 37° 10′ N. and 75° E. (4) That the Karambar valley, although difficult, is practicable for ponies.

DR. THEODOR MENKE, one of the best known of German historical geographers, died in Gotha in May last. His work in the compilation of atlases of historical geography was exceptionally thorough. His first work in this direction was a popular school atlas of classical geography, entitled "Orbis Antiqui Descriptio"; but his most important contribution to cartography was his edition of Spruner's great historical atlas, begun in 1858 and completed in 1879.

DR. STUHLMANN, according to a telegraphic report in the *Times*, has furnished additional particulars of Emin Pasha's expedition, although no more recent news. The real and only aim of Emin's journey to the Equatorial province, was to rescue those of his former subordinates, whose vacillation and delays kept them from joining Stanley's march to the coast. It was then his purpose to make his way across Africa to Adamawa and the Cameroons, a purpose which, as we already know, he had to abandon. It is satisfactory to learn that Dr. Stuhlmann had with him at Bukoba all the valuable scientific records and collections of the expedition.

THE current number of *Petermann's Mitteilungen* calls attention to a curious literary fraud to which in the two previous numbers it fell a victim, and from which many geographical journals in the habit of faithfully reproducing the articles of *Petermann* also suffer. A Dr. Ceyp professed to have made a journey recently in south-eastern Persia, and communicated to *Petermann* a detailed account of it, which now appears to have been copied verbatim from a little-known work, "Gasteiger-Khans," reprinted from the "Boten für Tirol und Vorarlberg," 1881. General Houtum Schindler, of Teheran, who knew that Ceyp's Persian travels had not led him beyond that city, gave the information which led to this discovery. The episode furnishes a fresh proof of the necessity for the great caution in accepting the records of unknown travellers which has always been exercised by the leading English authorities.

THE BEARING OF PATHOLOGY UPON THE DOCTRINE OF THE TRANSMISSION OF ACQUIRED CHARACTERS.

FOR more than two years the English public has been in possession of an excellent translation of sundry of Weissmann's more important essays.[1] The object of this paper is not to expound Weissmann's views generally. That office has already been undertaken by the persons best qualified to perform it.[2] We propose merely to discuss one of his topics under a single aspect—the "Transmission of Acquired Characters" in its relations to pathology.

We cannot, however, avoid reviewing some of the leading points in Weissmann's system which bear upon our immediate topic.

At the root of the matter lies the all-important distinction between reproductive and somatic cells. Saving among the lowest forms of animal life, an organism may be regarded as made up of two parts. There are the reproductive cells. With these the future of the species lies. They are the visible basis of its perpetuity. The remaining tissues of the body are styled "somatic." It is natural to us to think of the "somatic" tissues as something higher and nobler than the reproductive cells—to contrast the simplicity of the latter in structure and endowment with the intricacy of the former. But there is another point of view, which inverts matters; which regards the somatic tissues—the body and its manifold endowments—simply as a sort of living case or appendage of the reproductive cells. The reproductive cells look after the perpetuity of the species, the somatic cells look after the reproductive cells.

Now, if we travel back to the simplest forms of animal life, we lose sight of this distinction. The principle of differentiation of labour is not yet recognized. Among the Protozoa the distinction between reproductive and somatic cells has no place. Every part of the organism has it in its power to reproduce the entire organism. No special material is reserved to serve the purposes of reproduction. As we ascend in the scale of animal life, differentiation of labour begins. There is from the outset a reservation of reproductive cells, which serve as the demonstrable links between successive generations of organisms. But in sundry of the highest forms of animal life a third condition obtains. There is at the outset no reservation of cells: differentiation overtakes the entire organism—there is no exemption.

Not till the close of embryonic life do the reproductive cells appear, and when they do so it is as the offspring of somatic cells. This third condition was felt by Weismann as a difficulty, and led to an important modification in his terminology. The problem he had to explain was this, How can cells which have apparently lost their reproductive characters afterwards regain them? The solution he found was that the differentiation undergone by certain cells was never in reality thoroughgoing enough to deprive them of their original characters. Sooner or later, a moment arrives at which the original "germ-plasm" becomes again predominant. Instead, then, of in "germ-cells," the basis of perpetuity of the species is laid in "germ-plasm."[1]

We have now to consider the bearing of these views upon the doctrine of the transmission of acquired characters.

It is of the utmost importance to understand precisely what Weismann means by the term "acquired character." Acquired characters are opposed to original characters. To grasp the distinction we are sent back to a time before the distinction between reproductive and somatic cells existed. The characters already present at this early period are original characters. Later on, the reproductive and somatic cells part company, to follow separate careers of their own. It is the somatic cells—the body—which comes chiefly into collision with the environment, and in doing so undergoes various modifications. Now these modifications are the "acquired characters" the transmissibility of which Weismann denies.

They may be something purely local, as a scar or a mutilation. They may be something which involves the modification of complex musculo-nervous mechanisms, as in delicate manipulations and tricks of skill, such as violin-playing. Now, how is it conceivable, he argues, that such specific changes in the somatic tissues should influence the reproductive cells in the same direction? Whether they influence them at all is not the matter in dispute. That they do this is not only conceivable, but highly probable. But how can the somatic cells stamp their own special characters upon the reproductive cells?

We now turn to the main topic of this paper. Has pathology anything to say, either for or against, the transmissibility of acquired characters?

Now, as to the transmissibility of sundry forms of disease there is no question. That pathological characters are transmitted is universally allowed. The difficulty, however, is to decide whether such characters were really acquired, in the strict sense in which Weismann uses the term. We shall find that it will require considerable care to adduce instances which are really appropriate. With this preliminary caution we may proceed to attempt some sort of preliminary classification of our pathological data. We shall find that they fall, roughly, into three main groups:—

(1) Morbid characters which are obviously acquired by the organism, and as obviously transmitted. But since they are in no sense the acquisition of the somatic cells as such, but of the entire organism—somatic and reproductive cells alike—they cannot be allowed to "rank."

(2) Morbid characters in which an element of transmission is obvious, but where a closer investigation reveals the fact that, supposing them to have been acquired, in Weismann's sense of

---

[1] Translation edited by E. B. Poulton, Schönland, and Shipley.

[2] Prof. Moseley's two articles in NATURE, vols. xxxiii. and xxxiv. Discussion introduced by Prof. Lankester at the meeting of the British Association, 1887.

[1] See Weismann's essay on "Foundation of a Theory of Heredity," *passim*.

the word, it is not precisely what was acquired that is transmitted, but something broader and more general.

(3) The cases which are really in point: morbid characters which were really acquired by the somatic tissues alone. We shall see, later, whether or no these are transmitted.

(1) This group embraces all those cases in which a morbid character is acquired by the entire organism, somatic and reproductive cells alike. Behind the distinction between somatic and reproductive cells lies the fact of a common relation to the circulatory and nervous systems. Any change, therefore, in the circulation for example, will affect both. A pregnant woman takes a fever, and transmits it there and then to her offspring. There is no more mystery in this than in the fact that certain poisons produce abortion—indeed, the *materies morbi* is a poison in either case. But this explanation has, in all probability, a much wider range than the zymotic diseases. Consider, for example, gout. In a sense it is no doubt true to say that gout was an acquired disease. We can point to periods in the world's history in which gout was conspicuous by its absence. We can trace with some degree of accuracy its rise and progress at different epochs, and point to the conditions under which it rose, as, for example, in the early days of the Roman Empire.[1]

But even if we allow that gout was, in a general sense, an acquisition of civilized society, we have only to reflect on its pathology to see that it could never have been acquired in Weismann's sense. For what is gout? People usually think of gout by one of its manifestations—inflammation. This, however, is in reality no more than a symptom—perhaps than an incident—of a condition. The gouty attack is due to the existence of certain sites in the system conveniently cool and dry for the deposition of what are popularly known as chalk stones, if, indeed, it be correct to think of the morbid process as a deposition. The general morbid condition lies deeper, and still eludes us. But if we are in the dark as to the precise nature of the pathology of gout, it would be affectation to say that we are unable to prescribe its general outlines. Is it a degeneration, in which the entire organism shares? Then it will be a morbid acquisition of both somatic and reproductive cells alike. Or is it a failure in metabolism generally? The same will be the case. Or is it due to a failure in some particular gland to elaborate the materials brought to it, or to do its share of excretion? If so, the mischief will immediately make itself felt in the circulation, and the conditions of the sufferer will become practically those of slow self-poisoning. So that on no hypothesis can we represent gout as an acquisition of the somatic cells exclusively.

It is the element of progressive heredity which makes the hypothesis of transmission of acquired characters an attractive one in a disease like gout. This element is, in the case of certain families, strongly marked. We even see children suffering from the disease. And bearing in mind what we know of its ætiology, we naturally say to ourselves, "It was not this child's fault that he was born gouty. 'The fathers must have eaten the sour grapes,' or in this case, perhaps, have drunk the sweet ones." But it needs but a moment's reflection to convince us that the element of progressive heredity, so far from being an anomaly, is deducible from the facts of the case. It is true that here we cannot directly apply the theory of natural selection. We are not now concerned with conditions of progress, but with those of regress. Nature selects the fittest. There is no reason why she should select the goutiest. The question we have to ask in disease is not whether Nature selects, but whether she summarily rejects. If she stepped in and exterminated the gouty, she would stop gout altogether, and with it the feature of progressive heredity. But there is no reason to suppose that, as a fact, she does anything of the kind. In the first place, gout is not a disease which seriously shortens life; in an advanced stage of civilization its existence is quite consistent, not merely with life, but with the active discharge of elaborate duties.

But there is another more important consideration. Strange as it may sound, there may be good reasons for supposing that Nature, so far from rejecting, might even select, the goutiest. For gout, like other diseases, is only one corner of a much wider question. Diseases have coincidents and relations which stretch beyond the bounds of pathology, and trespass upon biology. This, indeed, is a side of clinical study which has only comparatively recently received its proper recognition. In former days men contented themselves with observing the morbid symptoms of a gouty patient; they paid no regard to his other "points"—his nails, his teeth, his intellectual endowments. But it may often happen that morbid characters have their good affinities. This is probably the case in gout. We have heard it said, for example, by one of wide experience in this disease, "No gouty person is a fool"—a statement which derives some support from the number of eminent men who have been the subjects of this disease. It is often implied that in what is termed an "artificial" civilization natural selection ceases. Might we not, perhaps, say that it still proceeds, only upon a modified plan. The conditions of the competition for existence have altered. The fittest in one generation need not be the fittest of another. Thus, in a rude state of society, in which sustained physical strength is the one thing needful, the gouty man would have no chance. His enemies, however inferior they might be, would have nothing to do but to lay by for the next attack of gout, when they would easily kill him. In a more advanced state of society all this is changed. If the gouty man has talents, he probably has friends and money. There is no demand for sustained physical strength. If he has the gout he can be nursed. His gout may be even of advantage to him—he gets into the papers. So that, paradoxical as it may seem, Nature may even select the gouty, not for their gout, but for their biological equivalents.

We have shown then that Nature, so far from interfering to exterminate the gouty, might even select them. But a more plain and obvious reason exists for the progressiveness which we sometimes observe in gout. If gout be a modification of the system generally, if its progressive increase in the tissues of a gouty patient with increasing years is in some cases a matter of observation, it would only be reasonable to infer that the same is true of the reproductive cells. For, if they share in the degeneracy, why should they not share in the progressive tendency? In the light of this consideration we can explain a fact widely received among medical men—that the incidence of a gouty inheritance falls mainly upon the younger children. Since the reproductive cells as well as the somatic grow goutier and goutier as age advances, the later their separation occurs the more likely will they be to manifest gout.

(2) The second group includes cases in which there is an undoubted transmission of morbid characters, but where it is by no means certain that they were "acquired" in the sense under discussion. But even if they were, it does not seem that what was acquired is transmitted, but something broader, and more general. We shall take as examples two important diseases—phthisis and "new growths"—alluding briefly to the phenomena of "short sight."

Phthisis may be said to be in one sense, like gout, a disease acquired by civilized humanity. "The naked savage," writes Dr. Andrew in 1884,[2] "whatever ills he may have to bear, rarely reckons phthisis among them; with every addition to his clothing and the comfort of his tree or cave, proclivity to it increases,"—a statement which is fully borne out by what we know of the spread of phthisis in the Rocky Mountains and the islands of the Pacific. If we know less of the history of the rise and spread of phthisis than we do of gout, we have more definite conceptions regarding its pathology. At the present day that pathology may be said to have two sides. There is the side originated and elaborated by Koch—the demonstration of the constant presence of a vegetable parasite in the tissues in this disease. There is the chemico-physiological side. Before Pasteur's time, such terms as "medium," "soil," as applied to the human organism, were little more than metaphors, while such words as "constitution," "predisposition," had little more than a metaphysical value. At present, scores of workers are busily engaged in translating these terms from the language of metaphysics into their chemical and biological equivalents.

If, then, phthisis was originally acquired, what was it that was acquired? It would seem that we can take our choice between saying that the microbe was acquired, or a habit of body favouring its growth. Supposing, then, the acquisition to have been no more than the lodgment of a parasite in the tissues, can we suppose that it is the parasite which is transmitted? Our facts will hardly warrant such an assumption. How, for example, could we interpret such familiar incidents as the following? A mother, after giving birth to several children, who successively fall victims to phthisis in young adult life, is ultimately attacked herself by the same disease, at a date removed by an interval of several years from the birth of the last phthisical child. Here we should be driven to assume, not in the case of the mother alone, but in each of the several children, a long latent period, during which the parasites, though present in the tissues, made no sign. Such an assumption presents great difficulties. Again, the direct transmission of tuberculosis from a mother to her fœtus is admittedly rare, whereas on the supposed hypothesis we should expect to find it common.[1]

---

[1] Pliny, "Hist. Nat.," lib. xxvi. cap. lxiv., ed. Franzius. Senecæ Opera, F. Haase (Lips., 1886), Epistul. Mor., lib. xv., Ep. 3 (95). Galen, "Comment. in Hipp. Aphorism," cap. xxviii., ed. Kühn, xviii. A. 42.

[2] *Brit. Med. Journ.*, 1884, 707

But if it is not the parasite that is transmitted, what *is* transmitted? We are driven back on the "other side" of the pathology of phthisis. But if we suppose that the transmission is not one of a parasite, but of a "diathesis," or "predisposition," then we desert the only standpoint from which there is any chance of proving that the disease was acquired in the sense under discussion. For what reasonable ground could we have for restricting this "predisposition" to the somatic cells alone, to the exclusion of the reproductive cells?

On the hypothesis that the thing transmitted is a "predisposition," we can, as in gout, explain the element of progressive heredity in phthisis. For, the admission of a morbid change once made, the difficulty is not so much to explain its progression as its arrest. In certain consumptive families we have in the limits of a single generation this morbid progress going on under our very eyes. It is the rule to find in such families, where several brothers and sisters are attacked, the younger fall victims at an earlier age than the elder, showing in this way their increasing liability. The explanation is probably identical with the one suggested in gout. The entire organism of the parent becomes more and more phthisically disposed—somatic and reproductive cells alike. The later the separation of the latter occurs, the more likely will they be to manifest phthisis.

The same line of argument is applicable to the facts of "short sight."[2] Short-sightedness is certainly hereditary—it runs in families—but that does not prove that we have in it an example of the transmission of acquired characters. For in the first place it would be very difficult to prove that the short sight was in the first instance acquired in the sense under discussion. While the progressiveness of the morbid character—which seems to support the theory—can be as well explained without it. For if there is no proof that the morbid character—the faulty build of the eye—is itself progressive, there is good reason to suppose that the habits of close attention which minister to the defect are so. In one generation we find a man simply tasking his eyes; his son works with a simple microscope; his grandson with an improved microscope.

I pass on to consider another group of pathological facts, of the highest importance and interest—new growths. The element of heredity doubtless obtains here as in the case of gout and phthisis. Thus the statistics of Sir J. Paget in this island, and those of Velpeau on the Continent, agree in showing that heredity can be traced in about one-third of the entire cases of cancer.[3] And among the benign tumours, as they are called, warts and exostoses are hereditary. Further, there is in some cases evidence of progressive heredity, the irregularity appearing in the children at an earlier age than it appeared previously in the parent. And we have here what might look at first sight more like a real transmission of acquired characters than anything we have yet dealt with. No one questions that something is transmitted. The theory of the local origin of the new growths is gaining ground everywhere, and might appear to carry the inference that they are acquired, and that no constitutional element is involved in them. Here, however, we must be on our guard against the fallaciousness of words. If by constitutional we mean something pervading the entire organism—a taint in the blood, and so forth—then there is little or no evidence to warrant our calling new growths constitutional. But if we mean, on the other hand, something which was represented in the original germ—an error in the original plan, not a supervening flaw—then there is nothing to encourage us in denying, and a good deal to warrant our asserting, their constitutional origin. However, such an admission is not necessary to our present purpose. Let us assume that they are acquired in the sense in which a scar is acquired. Is it a fact that what is acquired is transmitted? If so, we should look for identity in position and histological character in the thing transmitted. But on the whole neither of these conditions is fulfilled. Certainly they are not, in the case of cancer, as the analysis by Mr. Morrant Baker[1] of 103 of Sir J. Paget's cases clearly shows. The distribution of the cancers proper shows a variation within certain limits. There is a strong predilection for certain sites, but these sites are sufficiently numerous. Now, it often happens that, where several children inherit cancer from a parent, the growth appears in each case in a different site. Nor are the precise histological characters of the growth at all faithfully preserved in the course of transmission; while it has been often observed that on the bodies of cancerous people innocent growths exist as well.[2] So that the inheritance does not appear to be a liability to a peculiar modification in a certain part, but a tendency to one or more of a group of modifications in one of many possible sites.

Once more we find ourselves driven to a choice between two alternatives, either of which excludes the transmission of acquired characters. For if new growths are really acquired characters, then it is not exactly what is acquired that is transmitted, but something broader than it. If, on the other hand, they are only acquired in a more general sense, they fall outside the limits of Weismann's sense of the term "acquired character."

(3) There remain for our consideration the third, and, in one sense, the most important, group of pathological data—those which answer to the qualifications of acquired characters in Weismann's stricter usage of the term. Here, if anywhere, would be the ground in pathology to select for proving the theory of the transmission of acquired characters; but it must be confessed that this is just the region in which that theory receives the least support. This group of pathological facts embraces a number of accidental lesions, such as scars and mutilations, which are certainly acquired in the strictest sense of the word. But the evidence for the theory seems strong only in the dubious cases, weak in the unexceptionable ones. We have examples of mutilations practised for many centuries by entire races, without being transmitted in a single instance. Nor is it the experience of surgeons that scars and mutilations which are the results of operations are ever transmitted. On the other hand, we have histories of tailless cats and hornless cows. But here everything turns upon the comparative certainty with which we can prove that the initial lesion was really in the first instance acquired. Have we here to do with an accidental lesion or a deformity? A closer investigation has, in many instances, rendered the latter the more probable explanation of the two. For example, in the case of the tailless cats, closer research made it appear that the irregularity involved an abnormality affecting many of the lower vertebræ. In other cases, the abnormality in the child was so little like that in the parent, as to suggest that it was a merely accidental coincidence of two different lesions in one site.[3]

If we turn to the results of experimental research, we are confronted by more than one remarkable series of experiments, upon the bearing of which it is impossible as yet to pronounce decisively. The most notable work done in this direction is, perhaps, a series of experiments upon guinea-pigs, undertaken by Brown-Séquard, and repeated by Westphal.[4] They produced epilepsy in a number of these animals by various methods—section of the cord, section of different nerves, &c.—and observed subsequently that certain of the offspring were epileptic too.

But there are several reasons which prevent our accepting these results as decisive. The records of the experiments are said not to be very perfect. Then it is not contended that epilepsy was uniformly transmitted. What happened was that each member of the offspring presented some morbid symptom—usually some nervous trait, such as epilepsy or paralysis. So that the result of Brown-Séquard's experiments would rather seem to be this. By producing one morbid trait in the parents, he set up a liability to one of several in the offspring. By producing a single character, he set up a tendency. All this is of extreme importance, and it may well be that the future has much that is interesting to reveal in this direction. But, meanwhile, it cannot be said to lend very much direct support to the theory now under discussion.[1]

---

[1] See *Fortschritte der Medicin*, Bd. iii., 1885, p. 198; bacilli found in lungs of fœtal calf, æt. 8 months, whose mother was tuberculous.

[2] Ziegler and Nauwerck, "Path.," vol. i. pp. 393-94.

[3] Erichsen, "Surgery," seventh edition, p. 787.

[1] See "St. Bartholomew's Hospital Reports," 1866.

[2] Observation of Mr. J. Hutchinson, quoted in Fagge's "Medicine," vol. i. p. 106.

[3] For a number of other instances, see Weismann's essay on "The Supposed Transmission of Mutilations," *passim*.

[4] See Brown-Séquard, "Researches on Epilepsy," Boston, 1857; Papers in *Journal de Physiologie de l'Homme*, tom. i. and iii., 1858, 1860; *Archives de physiologie normale et pathologique*, tom. i.-iv., 1868-1872. Ziegler and Nauwerck, vol. i. p. 390. See also Weismann on Brown-Séquard, pp. 81, 310, 313; translation, edited by Poulton.

Again, the choice of lesion in these experiments was a somewhat unhappy one. Epilepsy is a symptom which can be produced in a number of ways—its proximate cause, if there be a single one, we are not as yet in a position to formulate. Attempts in this direction usually go no further than a vigorous and often highly poetical description, in which metaphors drawn from the phenomena of electricity are liberally employed. It might have been more advantageous to have aimed at the production of less equivocal symptoms, whose pathology is less disputed—such, for example, as facial palsy.

Lastly, we cannot exclude from these experiments the possibility of the introduction into the system of chemical poisons or even parasites, as incidental results of the operations.

But this does not by any means exhaust our stock of instances. The pages of pathology furnish us with more than one group of important facts which satisfy all the conditions of acquired characters.

Chief among these stand those numerous modifications of various organs which we regard, and rightly regard, from a clinical point of view, as part of a given disease, but which might perhaps be more correctly described as secondary adjustments made by the organism to meet certain primary morbid changes induced in different organs by the disease itself. Such, for example, is hypertrophy of the heart consequent upon valvular disease. Such hypertrophy is or is not a morbid symptom according to the point of view we happen to take. From the clinical standpoint it may be conveniently treated as part of the disease. From the biological standpoint it is an effort on the part of the organism to adjust itself to altered conditions brought about by the disease. It is certainly an acquired character, in the strict sense of the term.

An illustration will make this plain. Rheumatic fever is an hereditary disease.[2] Inflammation of the valves of the heart is common in rheumatic fever, and hypertrophy of that organ often follows as a consequent of this. But who would reckon hypertrophy of the heart as forming part of a rheumatic inheritance? It is true, no doubt, that whoso is heir to a disease is heir by implication to all the biological incidents of that disease. But he is not heir to them for the same reason. The one belongs to him as the inheritor of a morbid tendency, the other as the possessor of an organism. Diabetes, again, is in some cases markedly hereditary. Secondary characters are acquired in the course of this disease also; such as hypertrophy of the bladder or stomach. But, however doomed from his cradle to diabetes a peason may be, he is not born with an hypertrophied bladder and stomach. We should think it absurd that such accommodations as these should be made before they were wanted. If, then, we are right in regarding these as really acquired characters—and it is difficult to see how we can avoid so doing—it seems that pathology has here afforded us a sort of crucial experiment. Of the morbid characters of which sundry diseases are constituted, some are inherited, some are acquired—the one are constantly transmitted, the others, so far as we know, never are.

But no one pretends that every disease is inherited. Consider, for example, such a disease as lead-poisoning. Here, there is not, obviously, any element of heredity. That two people are not equally liable may be true enough; that predisposing causes exist is doubtless the case; but that does not prove an element of heredity. Predispositions may be themselves acquired, as is the case in alcoholism. In such diseases as lead-poisoning, we rightly stress the importance of the environment, and minimize inherited tendencies. But such diseases will be of little use to us here, unless two conditions are complied with. The first is that they leave durable and definite lesions behind them; the second is that such lesions are not inconsistent with the procreation of children. Of such lesions the familiar "wrist drop" of lead-poisoning may be cited as a good example. It is often durable; in not a few cases it is not cured; it is not inconsistent with the procreation of children. But there is no evidence to show that this or kindred lesions are ever transmitted. Facial palsy would be another instance, this malady being often of considerable duration. This group of cases constitutes another piece of negative evidence, not so important as the last, because these cases are rarer, but still not unimportant.

It can hardly be disputed that these characters are acquired in the sense under discussion. There must have been frequent opportunities of transmission, but we have no evidence of anything of the kind.

The general conclusion we have arrived at in this paper is that pathology, so far from offering any support to the hypothesis of the transmission of acquired characters, pronounces against it. We have seen that it is possible to bring up a mass of evidence, which seems at first sight to favour that hypothesis. On further consideration, however, it becomes clear that only a small portion of that evidence can be allowed to "rank."

A considerable number of facts must be rejected, because though there can be no doubt that the morbid characters here present are both acquired and transmitted, they are not acquired in the sense under discussion—that is, by the somatic cells exclusively—but by the entire organism.

A considerable number of facts, again, meet with a like rejection, because there is no question that here certain morbid characters are transmitted, yet even supposing them to have been acquired, it does not appear that precisely what was acquired is transmitted, but something broader and more general.

A considerable number of facts remain, which may be allowed to "rank" as genuine instances of acquired characters. These, if the hypothesis be correct, should be transmitted. But of such transmission we find little or no trace.

If we begin with scars and mutilations, even if the facts are not all on one side, the balance of evidence is decidedly against the hypothesis. If we appeal to the results of experimental research, the question is more open; but if the hypothesis does not encounter quite so decided an opposition in this quarter, it can scarcely be said to derive much support there.

If we pass into the main region of pathology, we have to use some circumspection in looking about for instances which shall be genuine examples of acquired characters. That such instances really exist it has been our endeavour to show, notably in those secondary characters which organisms acquire by way of accommodating themselves to the effects produced by disease. So far from being rare or recondite, these constitute a group of familiar and well-ascertained facts. If transmission has not occurred, it cannot be for want of opportunity—there must have been scores of such opportunities. That it has not occurred, constitutes a piece of very important evidence against the hypothesis under discussion. HENRY J. TYLDEN.

---

[1] For other instances of supposed transmission of morbid characters artificially produced, see Ziegler and Nauwerck, "Pathology," vol. i. pp. 391–92; Brown-Séquard's operations on eyes, Mason's on the spleen.

[2] "Treatise on Medicine," by Fagge and Pye-Smith. Third edition, vol. ii. p 694.

A TRIP TO QUEENSLAND IN SEARCH OF CERATODUS.[1]

My main object in going to Queensland was to procure, if possible, the eggs of Ceratodus and the creature itself; secondly, I wanted to collect earthworms; and, thirdly, to see the country. In my main object I was quite unsuccessful, for the simple reason that this year Ceratodus did not lay its eggs till late on in November—two full months later than the time recorded by the only observer who had up till then procured them. University work forced me to return, not by any means empty-handed, but without the one thing which had tempted me to go north.

To save time, and avoid unpleasantness also, I went by train. It is a long weary ride across New South Wales, especially in warm weather. Unfortunately I left Sydney by the northern mail on Friday evening. There were very few carriages, and some of what there were were "engaged" for legislators who travelled home free and in ease whilst we who paid for our journey were huddled and crowded together. This discreditable state of affairs seems to be common at the close of each week during the sitting of Parliament in Sydney.

The journey north leads by the side of the Hawkesbury River, and after passing across the well-known bridge the train skirts the shores of what appears to be a succession of lakes. In reality, the winding river, shut in by wooded hills, expands every now and then into sheets of water, each of which in the gathering darkness seemed to be a little lake. About eleven o'clock you find yourself apparently running along through the streets of Newcastle, and stretching out eastwards see the long quays and

---

[1] Paper read by Prof. W. Baldwin Spencer, before the Field Naturalists' Club at Victoria, on March 14. Reprinted from the *Victorian Naturalist* for June and July.